far more rapidly with a change of composition than do the other properties, diffusivity, tensile strength, and Young's modulus, which make up the thermal endurance of the glass. The coefficient of expansion of German laboratory glassware is something more than 10 per cent. lower than that of English, and this fact alone would account for the breakage of the English glass with drastic heat treatment.

There is no particular difficulty attached to the manufacture of glass having a low coefficient of expansion and high thermal endurance, and we may take it that the English manufacturers have decided that the maximum resistance to attack by reagents is to be desired, and have, accordingly, sacrificed thermal endurance to a small extent in order to obtain the encouraging results outlined in Mr. Jenkinson's letter in NATURE of October 28.

The whole question is largely one of general policy. If the chemists of this country prefer to use a glass of higher thermal endurance but with less resistance to reagents, then I have little doubt that the British makers would supply it. E. A. COAD PRYOR.

Milford, Park Road, Teddington, November 8.

The Separation of the Element Chlorine into Normal Chlorine and Meta-Chlorine, and the Positive Electron.

IN commenting on a letter under the above heading from Prof. Harkins in NATURE of April 22 last, I remarked that his assumption that "the hydrogen nucleus or the positive electron has, according to these papers, a weight, and presumably a mass, of 1·000, on the basis of oxygen as 16·000," was contradicted by experiment.

Prof. Harkins has pointed out to me that this assumption was made with the reservation "whenever the positive electron is combined in a complex atom," which I was careless enough to overlook. I wish, therefore, to apologise for my remark and withdraw it unreservedly.

I very much regret that this apology comes so late, but the delay is due to the fact that Prof. Harkins led me to understand that he was himself publishing a statement on the matter in NATURE, to which I could reply. He now tells me he has decided not to do so. F. W. ASTON.

Trinity College, Cambridge, November 9.

The Stereoscopic Appearance of Certain Pictures.

IN going round a picture gallery it will be noticed that in certain pictures the objects delineated appear to stand out in a similar manner to those seen with a stereoscope. A picture of this kind has the effect of making those surrounding it appear very flat by comparison. This appearance is not the characteristic of a particular artist, because in an exhibition, all the paintings being by one man, only one or two may be found which have this stereoscopic appearance.

The majority of the pictures will be correct in drawing, perspective, light and shade, but it will be noticed that this will be correct only for one eye—that is to say, the picture is quite correct for either eye when the other is closed. Those pictures, however, which have a stereoscopic appearance are painted so that the representation is as nearly as possible a delineation as seen by the combined retinæ, any disturbing element which would do away with this illusion being eliminated. For instance, a revolver pointed straight at a person so that the centre of the barrel is pointing directly between the two eyes will be seen quite differently with the two eyes. When the right eye is closed the left-hand side of the barrel will be seen, the right-hand side being invisible. When the left eye is closed the right-hand side is seen, and the left-hand side of the barrel is invisible. When, therefore, the barrel of the revolver is foreshortened and both sides are visible, the muzzle being pointed directly at the observer, the appearance is such as could be seen only with both eyes. The stereoscopic appearance is then very striking, and the revolver appears to follow the observer and to be pointed directly at him, no matter what position he takes up with regard to the picture. In all the pictures or portions of them which present this stereoscopic appearance it will be noticed that the appearance as seen by both eyes is represented, the left-hand side of the picture being represented as seen by the left eye, the right-hand side as seen by the right eye.

In a demonstration which I gave before the Physiological Society (*Journal of Physiology*, vol. xlviii., 1914) I showed that the perception of binocular relief is independent of double images and the stimulation of disparate points, provided that the object presents images to the two retinæ similar to those which are presented by an object in the field of vision. This can be shown by taking a pair of stereoscopic photographs in which the point of sight is at the centre of each and cutting them vertically in two, and then, having pasted the left half of the left photograph on the left side and the right half of the right photograph on the right side on white or black cardboard at an appropriate distance, so that there is no overlapping when placed in the stereoscope, a picture in striking relief is obtained when combined together in the stereoscope. In this case it will be noticed that there is no portion common to both fields of view. In each case the overlapping portion is combined with white. It seems probable that this is how binocular vision takes place in ordinary circumstances. If an object in high relief—as, for instance, a vase or the face of a person—be viewed at a short distance and one particular point fixated, it will be noticed that the right eye dominates the right side of the field of vision and the left eye the left side. The image seen is almost entirely that of the right eye for the right side and that of the left eye for the left side, as may be proved by noticing the relation of surrounding objects, and closing first one eye and then the other alternately. F. W. EDRIDGE-GREEN.

The Energy of Cyclones.

IT does not seem to me as though any really satisfactory theory has yet been put forward to explain the genesis and maintenance of cyclones; I fully agree with Mr. Deeley (November 11, p. 345) that they are not due to contiguous masses of air at different temperatures, but, on the other hand, I do not see how they can originate in an inert and stable region like the stratosphere.

Were storms produced by contrasts of temperature —or, in other words, by the so-called polar front—surely they would be most violent where the contrast was most marked. The stormiest parts of the world are the great belt of the southern ocean from 40° to 60° S. lat. and that part of the Atlantic which lies north-west of Scotland, and neither of these regions shows any exceptionally steep gradient of temperature.

Observations in the upper air have shown a remarkable uniformity in the mean temperature (mean with regard to height) from 0 to 20 km. in every place where they have been obtained, and it follows as a corollary that there is a very uniform pressure at 20 km. height over the globe, for the pressure at 20 km. is almost independent of the surface pressure.

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Observations over Europe, the only part of the world where they are numerous enough for the purpose, have also shown a most extraordinarily close correlation between the temperature and pressure of the air in the upper part of the troposphere, many of the coefficients exceeding 0·90. These facts must be reckoned with in any theory about the formation of cyclones.

My own belief is that pressure differences originate in the upper half of the troposphere from variations in the strength of the surrounding winds. Being given the means of originating and maintaining a difference of pressure at about the height of 9 km., the rest of the phenomena follow readily. The distribution of temperature, the high positive correlation below and the negative correlation above, and the rise and fall of the tropopause between cyclone and anticyclone are all explained by the vertical motion of the air that would naturally follow from the distribution of pressure. W. H. Dines.

Benson, November 12.

Physiological Method as a Key to the Causation of Isle of Wight Disease in Bees.

In the summer of 1918, while working with Prof. L. Hill and Mr. T. A. Webster on gas research, I advised Dr. John Rennie, of the natural history department of the University of Aberdeen, that a physiological study of metabolism in healthy bees and in bees known to be suffering from symptoms of Isle of Wight disease should be undertaken, making use of oxygen and carbon dioxide determinations. As a corollary, an investigation into the temperature of bees, healthy and unhealthy, was also indicated.

In the summer of 1919 it was found that apparently bees infected with Isle of Wight disease consumed in a given time much less oxygen than healthy controls. This observation was not reported to me until July, 1920, when I was able at once to draw the conclusion that the symptoms of Isle of Wight disease were due to blocking of tracheal tubes and anoxæmia. Loss of the power of flight was to be expected, as the co-ordinating nervous mechanism and the musculature involved were deprived of their proper oxygen supply.

Had I been made acquainted with the results of a research I had advised, or had the investigator been able to draw a very simple physiological conclusion from the data, whether the data were correct or erroneous, the discovery of a parasite as the blocking agent or disturbing factor in tracheal tubes would have been the natural consequence.

In 1920 this parasite was discovered by anatomical study, and its presence as the agent causing the disease is announced in the Press as the outcome of the researches of Dr. John Rennie, Mr. P. Bruce White, and Miss Elsie J. Harvey.

It will be noted that the physiological method of investigation could have led equally surely to the discovery of the parasite and to the interpretation of the symptoms. James M. McQueen.

Halesowen, November 5.

Luminosity by Attrition.

I am very glad that Sir Ray Lankester has again directed attention to this phenomenon. By a slip of the pen he says (November 4, p. 310) "quartz pebbles" in his first paragraph and in the heading. My object in writing is to say that these pebbles may be found on any shingle beach, and may be known by their translucent appearance under a "rotten" (or pitted) exterior. They are *pale* brown, and, when dry, look like lumps of derelict toffee. But on the shingle beaches of South Devon (notably the "Budleigh Salterton Pebbles"), the Chesil Beach, and the Suffolk coast (notably at Aldeburgh) practically every pebble on the beach will "flash."

It is much to be desired that some mineralogist should take up the intensive study of flint, chert, and quartzite. I may mention that for many years I have stored every "peculiar" flint pebble I have come across, and my entire collection is at the service (as a gift) of any museum worker who cares to undertake the study. Edward Heron-Allen.

Large Acres, Selsey Bill, Sussex,
November 12.

The recent letters in Nature from Lieut.-Comdr. Damant and Sir E. Ray Lankester recall some observations made in 1916 in collaboration with my friend and former headmaster, the late Mr. W. P. Workman, who first directed my attention to this interesting phenomenon. Specimens of translucent quartz from a quarry about three miles from Tintagel, North Cornwall, give the characteristic orange-coloured light in broad daylight and the peculiar smell. This *triboluminescence* of quartz was observed by Du Fay in 1735, and about 1748 Delius mentioned the sulphurous smell which accompanies the glow when quartz is rubbed against quartz (Kayser, Bd. iv., pp. 614, 617; Winkelmann, Bd. vi., p. 809).

The following observations present, I believe, some points of novelty: (1) Luminescence is produced when quartz is rubbed by any material, such as topaz or sapphire, which is harder than itself. Dr. Gordon, head of the geological department of King's College, London, kindly lent a number of minerals for the purpose of this test. Ordinary steel, which falls below quartz in the scale of hardness, does not cause the glow. (2) Fused quartz, as supplied by the Silica Syndicate, gives a very fine effect. I have on several occasions shown this as a lecture experiment by rubbing together two tubes of transparent fused silica. In this connection the observation of Lord Rayleigh (Nature, vol. civ., p. 153, 1919) that "silica glass" possesses a remarkable crystalline or quasi-crystalline structure is of special interest. (3) The tubes of fused quartz when rubbed together give the same peculiar odour as ordinary quartz pebbles.

It should, therefore, be possible to carry out an experiment of the kind suggested by Sir E. Ray Lankester by grinding the interior of a quartz vessel which might be highly evacuated or contain suitable liquid or gaseous reagents. According to Lord Rayleigh's experiments (Royal Society, February 27, 1919), the clearest and whitest quartz has some power of scattering light, though much less than that of glass or liquids. This small scattering is considered to be due to inclusions, as in the case of visibly smoky or yellow quartz. May not the presence of minute diffused metallic particles, or perhaps particles of silicon itself, be the cause of the various phenomena under discussion? H. S. Allen.

The University, Edinburgh.

Contractile Vacuoles.

There is no doubt that the explanation of the production of these vacuoles as given in Nature of November 11 by Prof. Henry H. Dixon is the correct one. It may perhaps be of interest to Prof. Dixon to refer to a paper by W. Stempell published in 1914 in *Zoolog. Jahrb., Abt. allg. Zool. u. Physiol. der Tiere* (Bd. 34, iii., p. 437), where the same view is put forward. I believe that this journal is not very accessible—a fact which doubtless accounts for its having been overlooked. I may say that it is my custom to teach this view, and a brief account will be found on p. 162 of my "Principles of General Physiology." W. M. Bayliss.

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